INTERVIEW OF C. N. YANG FOR THE C. N. YANG ARCHIVE THE CHINESE UNIVERSITY OF HONG KONG

Kerson Huang

(January 3, 2001)

Huang: This is Kerson Huang. It is July 29, 2000. We are in Professor Yang Chen-Ning's office at the Chinese University of Hong Kong. I am interviewing him on the subject of statistical mechanics.

Huang: Chen-Ning, do you think ergodic theory gives us useful insight into the foundation of statistical mechanics?

Yang: I don't think so. It is true that in the early days of statistical mechanics, there was much discussion about ergodic theory. In particular, it was very useful for mathematicians to launch into this subject; but in the 20th century, statistical mechanics mostly developed in the area of equilibrium statistical mechanics, and it has taken off really without much reference to ergodic theory. This does not mean that ergodic theory should not be pursued in the future, but I doubt that in the 21st century ergodic theory would have much influence on the development of statistical mechanics.

Huang: As you know, there's this Boltzmann factor

$$e^{-\beta H},$$

where β is one over Boltzmann's constant times the temperature. It turns into the quantum-mechanical time-translation operator

$$e^{-itH},$$

if we make the temperature pure imaginary by setting $\beta = it$. Do you think this is just a mathematical coincidence?

Yang: I think it is a coincidence. I myself do not see a deeper physical origin of this "coincidence," as you call it. But this is of course a deep subject. The path integral formalism of Feynman, which I regard as one of the great contributions to physics, indicates that the integral of the phase factor — that is, with the i — underlies the true meaning of quantum mechanics. It was not the way quantum mechanics was developed, but I think to understand what quantum mechanics is all about, the deepest way is through the path integral formalism, which deeply entangles this exponential phase factor. How that happens to be, in a formalistic way, related to the partition function expression. I do not see either a physical or mathematical significance, or deep meaning. It is of course very important, once one recognizes this relationship, to borrow from each other. It's a very deep subject.

Huang: Do you think we will some day have a theory of so-called "complexity" comparable to thermodynamics?

Yang: "Complexity" is a word which has been used and abused in recent years. There were people who thought that complexity will overwhelm the whole of physics. I do not think that is likely to happen. Now, complexity, as relating to such subjects like dynamics, is likely to be a very important subject, both for physicists and mathematicians. But I doubt it will have any importance comparable to thermodynamics.

On another level, complexity can have additional meaning, for example, in information theory and the structure of the human brain. These are mysterious subjects for future development. How that will play out in the 21st century I cannot guess. If you call that complexity, if you call the understanding of the structure of the human brain a part of complexity, then I would say that would likely be the one dominant subject of 21st century science.

Huang: Do you think a broader understanding of statistical phenomena will help us in understanding group human behavior?

Yang: I hope that we will have some penetration of the understanding of human behavior, both in the individual and in the group; but I think that subject is likely to be much more complicated then the subject of thermodynamics.

Thermodynamics was something that was very difficult for scientists to understand, and to tame. But once it has been understood in principle, and tamed, it has flowered into one of the major areas of understanding of physics in the last century. I doubt that the elegance and depth that characterize statistical mechanics of this century, the 20th century, is likely to be matched by the understanding of either individual human behavior or group human behavior in the 21st century. This does not mean that there would be no penetration. I'm only saying that I doubt the elegance that characterizes the statistical mechanics of the 20th century could be matched in that area in the next century.

Huang: I believe one of your earliest publications was on lattice models in statistical mechanics[1]. Can you tell us what attracted you to the subject?

Yang: When I was a graduate student in China in the wartime, 1942-44, I had studied with Wang Zhuxi on phase transitions, specifically, on such things as beta brass, which was a popular subject in the mid-thirties. So I was very familiar with lattice problems. Then, one day in 1944, I remember distinctly Professor Wang

[1]C.N. Yang, *J. Chem. Phys.* **12**, 66 (1945).

Zhuxi very excitedly told me that the two-dimensional Ising model had been solved exactly by a physicist named Onsager. He and I both studied Onsager's paper[2], but we did not get anywhere. However, the existence of that model, and the fact that it could be exactly solved, made a deep impression on me. That was the reason why I later got involved in statistical mechanics, in particular lattice models.

The involvement was not exactly smooth sailing at first, because, as I said a few minutes ago, in China in the wartime, I didn't understand what Onsager did. Later on, when I became a graduate student in Chicago, although Joseph and Maria Mayer were both there, they were not interested in phase transitions at that time. So I studied what Onsager did, on the side, by myself, and I still did not make much headway.

All that I learned from Onsager's paper was, he would commute A with B, and commute the commutator with B. And he was commuting everything under the sun. (Chuckle) I checked the computations, and they were all correctly done; but he didn't reveal the strategy, so you were led around by the nose, and suddenly the result pops out. That's not understanding.

But then, in 1949, during a van ride from Palmer Square in Princeton to the Institute for Advanced Study — a 15 minute ride — Quinn Luttinger, who was a fellow post doc at the Institute, told me about Kaufman and Onsager's simplification of Onsager's calculations[3]. That led me to an understanding.

After that, I like to tell graduate students that, if they launch into a field and get frustrated because they didn't understand it, that is not necessarily time wasted. In this particular example — my launching into the Onsager solution — the reason I could understand the key idea during the 15 minute ride with Luttinger was that I had "prelearned" the whole subject very well, though without true understanding. When the important point was revealed to me, I was able to immediately appreciate the whole strategy. That subject became one of my fascinations in physics, and that was truly the answer to your question.

Huang: You calculated the spontaneous magnetization of the Ising model, and obtained a very simple formula after some intricate mathematics, in what Dyson[4] called "an intricate Baroque music of elliptic functions." What motivated you to study this problem? Was it the belief the answer would be simple?

Yang: No, it was not because I felt the answer would be simple. The answer was amazingly simple, much simpler than what I had expected, and much simpler than what the intermediate steps had led me to expect. What happened was the following.

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[2] L. Onsager, *Phys. Rev.* **65**, 117 (1944).

[3] B. Kaufman, *Phys. Rev.* **76**, 1232 (1949); B. Kaufman and L. Onsager, *Phys. Rev.* **76**, 1244 (1949).

[4] F.J. Dyson, in *Chen Ning Yang, A Great Physicist of the Twentieth Century*, eds. C.S. Liu and S.-T. Yau (International Press, Boston, 1995), p.131.

Onsager and Kaufman, and earlier Onsager, had calculated the partition function, and the specific heat of course. The mathematical method Onsager used was very powerful. It not only gave the largest eigenvalue of the transfer matrix. It gave, in fact, all eigenvalues at the same time. Now, in calculating the partition function and the specific heat, Onsager needed only the largest eigenvalue. So I thought, why waste all this knowledge. Couldn't all these additional eigenvalues be useful? I immediately realized that to calculate the spontaneous magnetization, you need only the wave functions with the two largest eigenvalues — not just the largest, but the next largest one too. So I said, this seems to be a good problem, and I began to calculate that.

Actually, there's a bit of story before that. I told you earlier that Luttinger told me the main idea of the Kaufman-Onsager thing. When I got to the Institute, I spent a couple of hours, putting aside what I was doing in elementary particle physics, and pushed through that idea, and understood thoroughly the Kaufman-Onsager method. Then I said, well, why don't I collaborate with Luttinger to do a similar calculation with, let's say, a triangular model. That afternoon I talked to Quinn, but he was at that time doing some renormalization calculations, and he said no, he did not want to do it. So I thought about it a little longer, and decided it was a copycat thing, not challenging enough. So I put it aside.

More then a year later, after I had a rest from what I was doing in elementary particle physics, I came back to this model. As I said, I realized that to calculate spontaneous magnetization, all you need are the property of the largest two eigenvalues and the wave functions. So I got launched into that.

Huang: Was your method closer to Onsager's original method, or Kaufman's method?

Yang: Kaufman's. Onsager's original method was opaque. Very few people later refer to Onsager's original method. Anyway, — I remember this was in 1951, around January or something — it was a tortuous mathematical problem, both frustrating and enticing. Frustrating, because every other day you would find the problem absolutely hopeless; but after you think about if for a couple of days, you found you can turn the corner, and launch into a new direction. This repeated itself many times. Of course, the frustration sometimes led me to believe it was a totally useless exercise, and I would give up. But after a couple of days, something always happened to make me feel I could strike out on a different route. This went off and on, off and on, for about half a year.

During that process, there were a lot of elliptic functions. I had learned that elliptic functions was a beautiful subject, when I was a graduate student in China. I never thought it would be useful in a physics calculation. I was of course delighted that elliptic functions entered into the calculation, but the thing that really surprised me was that all the elliptic functions dropped out in the end. There were a lot of elliptic functions, and they were both in the denominator and numerator. I calculated the numerator — lots of elliptic functions. And I calculated the denominator — lots of elliptic functions. And when I put the two together, they all dropped out, and I got an algebraic expression of great simplicity[5].

As a matter of fact, the algebraic expression was so simple that it led to a further development. A year later, T.D. Lee and I got onto the subject of phase transitions. We began to guess at other algebraic expressions that might solve the Ising model in an imaginary magnetic field, and we guessed correctly. How do we know we guessed correctly? Because after our guess, we calculated the series expansion, and found that the results agree with the series expansion to — I forget — ten terms or something like that. So we asserted that this is the right expression. That guess was finally proven some 10 years later by T.T. Wu and McCoy[6].

To answer your question directly, I did not expect the results to be simple. And in the process of that happy calculation, I did not believe it would come out with a simple answer.

Huang: I guess this effort had something to do with the later work that you and T.D. Lee did on the general description of phase transitions, based on the distribution of the zeros of the grand partition function in the complex fugacity plane.

Yang: The beginning of my work with T.D. Lee was indeed a direct consequences of that magnetization calculation.

In the summer of 1951, after I arrived at that simple result — I still remember that was a very hot summer — Chi Li and I were in Urbana, Illinois, where our first son Franklin was born. I wrote that paper during the first month of Franklin's life. Then we went back to Princeton, and T.D. Lee joined me from Berkeley.

T.D. got his PhD in Chicago in the summer of 1950, approximately. But actually he had finished his thesis before that, and Fermi had arranged with Chandrasekhar for T.D. Lee to go to the Yerkes Observatory to work with him. Now, T.D. Lee's thesis work with Fermi was in astrophysics. So it was natural for him to work with Chandra. He wrote several papers while he was at Yerkes. He was there for only half a year, but he didn't get along with Chandrasekhar, so he went to Berkeley.

I still have a copy of a letter I wrote to Wick, who was then the chief theorist at Berkeley, recommending that he accept T.D. as a Post Doc. In the summer of 1950, T.D. arrived at Berkeley. But, unfortunately for him, it was that summer when there was a big exodus from Berkeley, because of the loyalty oath business. Wick left for Carnegie-Mellon — no, I forgot where in the city of Pittsburgh. So T.D. arrived at Berkeley and found there was no one to talk to. So between 1950 and 1951 he was unproductive and unhappy there. I think he might have written a couple of short papers, winding up his affairs in astrophysics; but he was frustrated, as far as elementary particle physics was concerned. He wanted to come back to

[5]The answer is $\left[\left(1+x^2\right)\left(1-x^2\right)^{-2}(1-6x^2+x^4)^{1/2}\right]^{1/4}$, where $x = e^{-2\epsilon/kT}$, ϵ being the attraction energy between neoghboring spins.

[6]B.M. McCoy and T.T. Wu, *Phys. Rev.* **155**, 438 (1967).

elementary particle physics. So in the spring of 1951 I asked Oppenheimer to invite T.D. to become a postdoc at the Institute. Oppenheimer readily agreed, and that was how, in September or August of 1951, T.D. arrived from Berkeley.

We became neighbors — our apartments were next to each other. Of course, we immediately launched into an intense collaboration. Now, I had just finished the paper on magnetization. Therefore it was natural to do the next calculation, the susceptibility. During the first two or three weeks, T.D. and I launched into the susceptibility calculation. That requires a second-order perturbation calculation, rather than a first-order perturbation calculation, as in the magnetization case, and now requires the eigenvalues of all the eigenvectors. After three or four weeks, we realized this was an impossible task, because just to calculate one matrix element took me half a year. To calculate the sum of all of this was absolutely impossible.

But in the process of doing this, we found something very interesting, by toying with very small samples. Instead of a large lattice, we started with very small lattices — three sites, four sites, five sites — and we formulated the problem in the simplest algebraic language possible, and we got polynomials. Pretty soon, we notice that these polynomials have roots on the unit circle, if you think of the fugacity as a kind of variable. That's how we found the unit-circle theorem.

I remember after we discovered the first few cases of the unit-circle property, we launched into bigger and bigger lattices, and in every case the unit-circle theorem held. So we got very excited, and said this may be true in general. So we went to Selberg — a number theorist, a very distinguished mathematician — and we went to Johnny von Neumann. The reason we talked to Selberg was that the number theorists have sort of a predilection for roots on the unit circle.

I still remember what von Neumann told us, after we explained our guessed theorem, that this type of polynomials always have roots on the unit circle. Even though he was infinitely bright, he had never tackled such a problem. It's rather a special problem for him. I remember him telling us,

"Well, to have a condition for roots on a unit circle, you can consult a book called *Inequalities* by Hardy."

So we got the book[7]. We found that, for the roots of a third-degree polynomial to be on a unit circle, there are three inequality conditions to be satisfied. For a polynomial of the fourth degree, there are four inequalities, and for five degrees there are five inequalities. We checked that, indeed, for small numbers of sites, these inequalities are all true. But of course that's very far from proving it for a polynomial of degree n. (Chuckle) After struggling with that for a couple of weeks, we found Hardy's book totally useless for our purpose.

Fortunately, Marc Kac was at the Institute that year, and he was intensetely interested in statistical mechanics. We told him about the problem, and he came back one day, and said that for a very, very simple example, he can prove the theorem. What was the example? Let me see. I don't remember now, but it's something like:

[7]G.H. Hardy, *Inequlities*, 2nd ed. (Cambridge University Press, Cambridge, Egnland, 1952).

If you had n sites all interacting with one another, so that there are $n(n-1)/2$ interactions all of the same strength, then you can prove the theorem. Something like that. We checked that he was right. But that was useless.

Finally, one day — I remembered distinctly a few days before Christmas — I found the key. The key is to loosen the proof that Kac figured out for that particular example. I vaguely remember it was something like this: If all the interaction strengths are the same, then you can prove it. How about relaxing that condition, and make them slightly different, and then make them very different? Along that route, I began to introduce many variables rather then one variable, and, playing with that idea, eventually completed the proof.

We were of course very delighted by this. It was an elementary proof. When it was written up, I think it was half a page in the Physical Review[8] But since it was such an elegant thing, it gave great insight into this type of phase transitions. So then Lee and I realized that we had some general ideas about phase transitions, which have been puzzling physicist ever since 1938[9]. In 1938, Joseph Mayer produced a theory of phase transitions, and after that there were many learned papers trying to discuss phase transitions in terms of the convergence of the virial series. After Lee and I had proven the unit-circle theorem, we went back to that discussion, and realized the earlier discussion were defective, because they were not careful enough about the switching of the limits. The switching of the limits became crystal clear with the unit-circle theorem.

Huang: You mean the dependence of the cluster integrals on volume and temperature? You have to keep the volume finite to discuss phase transitions. Is this what you mean?

Yang: Yes, there are two things you have to do. One is the limit of each cluster integral as the volume becomes large. The other one is the sum of the series. You have to be very careful about the order in which you do it, otherwise you reach incorrect conclusions[10]. We were very pleased, and I think the general statistical mechanics community was very pleased, that this completely clarified something that had been a bit murky for quite a number of years.

By the way, you didn't ask the question, but I should say something here. While I was doing statistical mechanics, and later on while I was doing the thing with T.D. Lee, and got a result which everybody felt was very beautiful, I instinctively felt, as a young physicist — at that time I was no longer a postdoc. I had a five-year appointment at the Institute — I instinctively felt that, although Oppenheimer

[8]C.N. Yang and T.D. Lee, *Phys. Rev.* **87**, 404 and 410 (1952). The proof actually occupies a page and a half, in an appendix. The paper itself is much longer. Yang has descibed in some detail the history of these two papers in *Selected Papers, 1945–1980, with Commentary* (Freeman, San Francisco, 1983) p. 14-16.

[9]C.N. Yang and T.D. Lee, *Phys. Rev.* **87**, 404, (1952).

[10]The virial series for the pressure is of the form $P = \lim_{V\to\infty} \sum_{\ell=1}^{\infty} b_\ell(V) z^\ell$, where b_ℓ is a cluster integral, and z is the fugacity. The limit $\lim_{V\to\infty} b_\ell(V)$ exists, but the limit $V \to \infty$ and the summation do not commute, when there is a phase transition.

did not say anything, he disapproved. I instinctively felt that he felt these were unimportant things. Why waste your time on these things. Why not come back to elementary particle physics, which he regarded as important. He never said anything to me. I do not remember him ever saying anything to me, but as I said, I could feel it.

Huang: Maybe the way you did it was not his style. I thought he was very much interested in the theory of electrons in metals that Bohm and Pines were doing.

Yang: Yes, he was interested in solid-state physics. But what I was doing, and what I was doing with T.D. Lee, was (Chuckle) more mathematical then Oppenheimer's taste would allow. At conferences I would overhear him saying to somebody,

"Frank has done a beautiful piece of mathematics."[11]

As I said, I could feel his disapproval, but not prove it. (Chuckle)

I would like to tell this story to young physicists, because it was a universal feeling. Everyone has a department head, and every person feels his department head is not exactly approving of what he is doing, and so he undoubtedly feels a certain pressure. (Laughter) Whether Oppenheimer approved or not, he appreciated my work, so therefore I had no problems with promotion at the Institute. (Chuckle)

Huang: On this subject, do you think it worthwhile to pursue the circle theorem, by classifying phase transitions according to some standard sets of distribution functions of zeroes? For example, can you classify universality classes in terms of distribution functions, and obtain critical indices?

Yang: I think many people have thought about this, and there have been many papers on the roots of polynomials, some of them with complex fugacity, some of them with complex temperature. Especially after the computer, you will find many many papers, if you look into what is called the computer physics literature. As far as I know, no useful discussion of the universality class and the characteristics of the root distribution has emerged. But very beautiful and very interesting results and conjectures have appeared, and this type of work still continues vigorously today.

Maybe we should check whether ... (Yang suggests checking the recording tape.)

Huang: Einstein had asked you and Lee for a personal exposition on your theory of phase transitions. You had told me that, while you were deep in the complex fugacity plane, Einstein asked,

"How do the molecules know?"

How did you answer that question?

Yang: I don't remember very much about what Einstein was asking, and I'm a little surprised that I told you, after we came out of Einstein's office, that Einstein

[11] Yang's friends and colleagues have called him "Frank" since his graduate-school days at the University of Chicago.

kept on asking how do the molecules know. So I don't know how I answered. The question "How do the molecules know?" did not originate from that time. It was explicitly down on paper in the conference report of the Van der Waals Conference of 1938 in Amsterdam. I said earlier that, in 1938 there was a paper by a young physicist or chemist Joe Mayer on phase transitions. That was the first discussion of phase transitions beyond mean-field theory[12].

Before 1938, all phase-transition discussions, including what I did with Wang Zhuxi in China, involve what we now call mean-field theory. But in 1937 or 1938, Joseph Mayer published several papers, and did not use the mean-field concept. That caused a great stir, as you can gather from the proceedings of that conference. I think they were published in the Proceedings of the Royal Society of London.

Let me back up a bit. Before 1938, in thermodynamics, there was this liquid phase, and there was another phase, which was the gas phase. Although made of the same molecules, they were rendered differently. Therefore the phases had nothing to do with each other. The liquid phase had its free energy, and the gas phase had its free energy, and when you match these free energies, or whatever, you get equilibrium — you get the phase transition. That kind of idea, borrowed from thermodynamics of the last century — borrowed from Gibbs — was sort of the background idea.

But in Mayer's work, he had only one partition function, and that partition function works both for the gas and the liquid. Therefore people ask the question,

"How did the molecules know whether they should behave as gas or liquid?" (Chuckle)

It's possible Einstein was influenced by that. It would be interesting to search through Einstein's papers, because Einstein's papers are in the process of being published, — but they haven't gotten to the thirties yet. It was well known that one of his main thrust in understanding physics was based on statistical mechanics.

Huang: I thought Einstein invented the partition function independently.

Yang: Right. So it could be that, in his correspondence in the late 1930's, he was very deeply immersed in this thing, and therefore he would ask Lee and me this question. I don't remember him asking me that. Furthermore, — let me see — I may or may not know about the 1938 Van der Waals conference when I went in to see Einstein.

Huang: There was also a person named Ursell, of the Ursell-Mayer theory.

Yang: Yes, that's right. Ursell[13] was before Mayer, but Ursell did not use his calculation to discuss phase transitions. I think if you check Mayer's papers, his ideas originated from many things Ursell did.

[12] J.E. Mayer and S.F. Harrison, *J. Chem. Phys.* **6**, 87 (1938).

[13] H.D. Ursell, *Proc. Camb. Phil. Soc.* **29**, 685 (1927).

Huang: Well, I always thought that the molecules did not know, and need not know; that the phase transition is a collective behavior, hardly noticeable to the participants. Do you agree with this?

Yang: (Long pause) Er — I would agree. The onset of a phase transition is a very complicated collective phenomenon. An atom might feel

"Whoops, we are going through a phase transition, "

because of his perception of the surroundings. In particular, the density may be rapidly changing. But if you make a phase transition an infinitely sharp thing, then he wouldn't know.

I may be wrong, my vague recollection was that Max Born had asked the question "How do the molecules know?" at that meeting. There had been the entrenched idea that each phase has a different partition function. Today it sounds ridiculous why it isn't obvious there should be one partition function; but I guess concepts in physics develop this way. If you start in error with certain general assumptions, sometimes you are not aware that there are these assumptions, and when these assumptions do not turn out to be right, you are shocked.

Huang: I think perhaps it comes back to what you mentioned earlier, about taking the limit of infinite volume.

Yang: Yes, it is deeply related to that.

Huang: Well, I think that's a fascinating question, because you know that in society there are revolutions. How do individuals know? They don't know, (Yang chimed in unison:) and yet they appreciate it.

Yang: You might say different people appreciate it differently. You remember the famous report made by the young Mao Zedong, on the peasant movements in Hunan, out of which came such famous sentences as "A revolution is no invitation to dinner"? (Yang quoted this in Chinese.)

He said in that report — that was a beautifully written report, from the point of view of being convincing —, we have two routes: Either we proceed to lead the peasants in the revolution that is to come, or we decide to oppose it. He was perceptive. Undoubtedly there were many other people who were less perceptive, and took the second route. (Laughter)

Huang: Kirkwood conjectured sometime ago that a classical hard-sphere gas has a phase transition. Computer simulations seem to indicate there is a first-order phase transition, arising from jamming at a smaller density than close packing. Do you think such a transition exists?

Yang: Yes, I do. I was not so convinced many years ago, but I looked at some of the computer calculations, and I'm convinced that the Kirkwood conjecture was correct. However, it might be very difficult to prove that statement. If you look at the calculations with bigger and bigger samples, I think it is very easy to be convinced that there should be a phase transition at a density close to, but not as large as, close packing.

By the way, do you know that Kepler's conjecture is probably proven now by Wu-Yi Hsiang?[14]

Huang: Which conjecture is that?

Yang: The statement that any close packing of hard spheres has the density of that of the face-centered cubic lattice.

Huang: Whose original conjecture was that?

Yang: Kepler.

Huang: Kepler the astronomer?

Yang: Yes. Kepler was deeply geometrical. He played with all kinds of geometrical things, including tiling, and duality. This was one of his conjectures. Mathematicians have been fascinated with this for several centuries, but it is only recently that Wu-Yi Hsaing wrote a book about it.

Huang: I see. Is this published already?

Yang: No, here's the manuscript. You may want to take it. I was going to throw it away, because I do not have space. You may scan it. The reason the problem is so difficult is because ....

If I ask you about close packing in two dimensions, you would immediately know the answer. You put one coin here, and then you put six coins around it. You easily find that there is no way you can pack them any closer, and so it becomes a triangular lattice.

In three dimensions, you put one ball here, and you can easily put twelve balls around it, but the twelve balls are not touching each other. Your intuition tells you that this face-centered cubic arrangement is not the best, because the twelve balls are arranged sort of loosely around the center one. If you now crowd them onto the top, you have a big hole in the bottom; but the big hole is not big enough to fit the thirteenth ball. The thirteenth ball can be quite close to, though not exactly touching, the center ball. So you would think that is the best way to do it.

I actually tried it many years ago. You put the thirteenth ball half way up, not quite touching the center ball. Then you try to arrange the next one. The principle is to make them as compact as possible each time. The difficulty with this method is that, after a while you have so many balls, and there are so many different possibilities, that it becomes impossible. That is the reason why for several centuries the problem was never solved.

The key idea of Hsiang is that he needs only to understand it up to three layers. The earlier difficulty is that you don't know at what layer you could stop, and it

[14] W.-Y. Hsiang, *Int. J. of Math.* **4**, 739 (1993).

was endless. He first proved a theorem that he only needs to analyze first two layers. After he analyzes the two layers, he says he has proven Kepler's conjecture for the infinite system. There is an inequality in there that I vaguely understand. I think it is a very clever idea. He may have solved that problem.

Huang: Coming back to the Kirkwood theorem, if you think it is true, then it reduces to a very well-defined mathematical problem, which ought to be provable. For example, the roots of the grand partition function should pinch the real axis.

Yang: Sure, sure, it's a well-defined mathematical problem. That does not mean that it's necessarily provable. (Laughter)

Huang: Once it is formulated, perhaps it can even be proven by computer?

Yang: You would need billions of computers to show a very sharp transition, and yet that is still not a proof.

Huang: No, no. Use a computer to find the roots of the grand partition function, to see whether they approach the real axis.

Yang: With modern computers, and especially using Padé approximants, you can make miraculously accurate predications. Nevertheless it's not a proof.

Huang: You originated the concept of off-diagonal long-range order (ODLRO). Is that the same as broken symmetry?

Yang: (Long pause) Mmm — I wouldn't say it's the same, though it's related. In the first place, the ODLRO was already implicit in a paper by Penrose and Onsager[15]. That's not the famous Penrose. I forgot his initial. It's a different Penrose.

Huang: Oliver Penrose, his brother.

Yang: Yes, his brother. They only applied it to bosons. When I was working on flux quantization with Nina Byers, we dealt with fermions, of course. After the Byers-Yang paper[16], I began to look into this matter, and realized that the boson idea of Penrose and Onsager, which is quite simple, can be generalized to fermions, and that is a much more complicated, and in some sense a deeper idea.

That was the origin of the ODLRO paper[17]. It is a little bit like the statement "Some atoms, under the right circumstances, would arrange themselves into a crystal."

In fact, I called that diagonal long-range order in my ODLRO paper. In quantum mechanics, there can exist diagonal long-range order, and there can exist off-diagonal long-range order. Aspects of ODLRO are strange to human perception. That's why "super" phenomena are so fascinating and so strange.

Huang: You seem to be saying broken symmetry is a more general concept.

[15] O. Penrose and L. Onsager, *Phys. Rev.* **104**, 576 (1956).
[16] N. Byers and C.N. Yang, *Phys. Rev. Lett.* **7**, 46 (1961).
[17] C.N. Yang, *Rev. Mod. Phys.* **34**, 694 (1962).

Yang: Yes, I would say so.

Huang: I recall you once said, broken symmetry is useful only when it leads to a renormalizable field theory. However, renormalization is not an issue in non-relativistic problems, such as ferromagnetism and Bose-Einstein condensation. Do you think the idea of broken symmetry is still useful?

Yang: I think the statement you recalled was made in the context of the use of broken symmetry in field theory.

Huang: The standard model?

Yang: Yes. In general, I would say that broken symmetry, which is a very general concept, is not related to renormalization.

As for broken symmetry and elementary particle physics, I think the story is not finished. Yes, we have a standard model, and there is broken symmetry, and that leads to a renormalizable field theory. These are very important developments, and amazingly they are in agreement with experiments. But I for one, and I think many people share my opinion, that it's not the final story. I have no idea what fundamental future developments are needed, but I'm convinced that's not the end.

Huang: So perhaps you feel the so-called Higgs field is not a fundamental field?

Yang: There are many proposals on what to do, but I did not find any proposal particularly beautiful and natural. So I think that is one of the open subjects.

Huang: In your work with Byers on superconductivity, you explained flux quantization in terms of the supercurrent necessary to minimize the free energy. Is this a general mechanism to drive the system to off-diagonal long-ranged order?

Yang: I wouldn't put it that way. Let's recall a little of what happened before 1961. Way before 1961, Onsager and London separately had conjectured flux quantization in superconducting rings. Now, I had written that that showed great physical insight. Nevertheless, their argument was wrong.

If you read London's books *Superfluids*[18], in one of them he discussed flux quantization before it was discovered. It's amazing that he and Onsager got into that; but if you read it, you realize his reasoning was totally wrong. As Bloch had later pointed out, if London's ideas were right, then all matter at any temperature would show flux quantization, because his argument had nothing to do with superconductivity.

The important contribution Nina Byers and I made was that it is only because of pairing, which is characteristic of superconductors, that there is flux quantization. In fact, there is great confusion in the literature. Most people have not understood this point. I believe this point was not understood by Sakurai in his textbook[19].

[18] F. London, *Superfluids*, Vols. 1 and 2 (Wiley, New York, 1954).
[19] J.J. Sakurai, *Advanced Quantum Mechanics* (Addison-Wesley, Reading, MA, 1967).

Now, what kind of a system could lead to ODLRO, and therefore to flux quantization? That was what drove me, after 1961-62, to go to the lattice models again, to try to find a system where I could reasonably argue that a fermion system would have ODLRO. So my later work with the Heisenberg model and the Ising model, resulting in what was later called the Yang-Baxter equation, was all derived from that main theme.

Huang: I don't know if you agree with the following view of broken symmetry. Given enough time, the spin system will sample all configurations. So, averaged over a long time, the total spin would be zero. Spontaneous magnetization can occur, however, because once the spins are aligned, it would take an exceedingly long time for the total spin to flip. From this example, one can say in general that broken symmetry arises from a failure of ergodicity.

Yang: Yes, I would say the spirit of this is correct. The first half of your sentence refers to a finite system. If you have a finite system, then over a long time, all configurations would be eventually sampled, and therefore the average spin would be zero. However, if you take an infinite system, and give yourself not an infinite time, then that statement is not true, and that is the spirit of broken symmetry. In this sense, broken symmetry, ergodicity, and finite and infinite systems, are all entangled concepts.

Huang: You must have given much thought to the profound role played by the quantum phase, from Bose-Einstein condensation to the non-integrable phase factor in electromagnetism and Yang-Mills theory. Would you care to comment on this subject?

Yang: Yes. You may not know that last year I gave a talk, the written version of which has not been finalized, entitled "Quantization, Phase Factor, and Symmetry — Thematic Melodies of 20th-Century Physics".

I believe what characterize 20th-century physics, so as to distinguish it from the flavor of physics in past centuries, are these three concepts.

Quantization, phase factor, and symmetry are deeply entangled. Very interestingly, all three concepts are contained, captured, in the Feynman path integral expression. (Chuckle) It has the phase factor; it has symmetry, especially gauge symmetry. And of course quantization, in the appearance of $\hbar$.

In talking about this, I'm proud of a paper that T.T. Wu and I wrote, but somehow very few people have noticed. In the seventies, we published a paper about the Dirac magnetic monopole quantization. You remember that the quantization says eg has to be quantized, for the theory to be consistent[20]. We asked the question, can we formulate a classical theory of electrons, monopoles and the Maxwell field? And how does monopole quantization enter into that. It turned out to be possible,

[20]P.A.M. Dirac, *Proc. Roy. Soc.* A**133**, 60 (1931) proposes the quantization condition $2eg/\hbar c =$ integer, where e and g are respectively the electric charge and the magnetic charge.

and we wrote a paper. The paper is not easy to read. It is a bit complicated mathematically, but it is extremely elegant.

Huang: Where was it published?

Yang: I think Physical Review[21]. It is in my collected papers[22]. The thing I want to point out is (Chuckle), there's no $\hbar$, so there's no Dirac quantization. You cannot have the Dirac quantization rule, if you don't have $\hbar$. But the quantization rule is already there. Why? We found something very interesting. We found that the action integral for such a system cannot be defined absolutely. It can only be defined modulo $4\pi eg/c$. When you put that into the path integral, with

$$e^{i\mathrm{Action}/\hbar},$$

you have to have the quantization rule in order for the path integral to be unique. I think that this statement has depth, but nobody has picked up on that.

(Long pause. Yang rummaged through *Selected Papers* to find the paper, and showed Huang the date.)

Huang: Yes, 1976.

Yang: At the end of the paper, there is a discussion about how that gets connected with the path integral. That, I believe, is the first example of a classical theory were the action cannot be defined, except modulo something. Later, one of your colleagues, Jackiw, produced some other examples[23]. But all those examples are less interesting, because they are not of the right dimension. Ours was the first and most interesting one, because it is electrodynamics.

Huang: You are right that this has not received wide attention. Maybe because there is not yet any application?

Yang: Yes, of course. But there is another reason. People follow Dirac, and Dirac has tackled the same problem, and didn't reach the same depth, because he always had a "string" — you know, the Dirac string. And then came Zwanziger. Zwanziger did not understand this, and his papers are wrong[24]. You know Zwanziger?

Huang: Yes, by name.

Yang: At NYU.

Huang: Yes.

Yang: In my opinion he didn't get it. He has both an A field and a B field describing the same electromagnetic field. He has too many degrees in freedom, and would have two kinds of photons.

[21] T.T. Wu and C.N. Yang, *Phys. Rev.* D**14** 437, (1976).

[22] C.N. Yang, *Selected Papers, 1945–1980, with Commentary* (Freeman, San Francisco, 1983).

[23] R. Jackiw, *Comments Nucl. Part. Phys.* **13**, 14 (1984). Called "Chern-Simons field theory", these models have found applications in the quantum Hall effect.

[24] D. Zwanziger, *Phys. Rev.* **176**, 1489 (1968).

Huang: Coming back to this point about the phase factor being an important part of 20th-century physics, it's interesting that it took almost a century for people to realize that the phase factor is the unique thing about quantum mechanics.

Yang: No, it is only important for the 20th century because of the $\hbar$. The phase is of course well known, but that's not real quantum mechanic. When quantum mechanic came along, it became fundamental.

Huang: Because of the wave function?

Yang: Yes

Huang: Because we add amplitudes?

Yang: Yes

Huang: But that was not the center of discussion at the Copenhagen School.

Yang: Because there was no Feynman yet. (Chuckle) Yes, quantum mechanics was not discovered through the phase factor, and in fact, you know, Schrödinger — did you read my paper about the history? — Schrödinger didn't like the i. Schrödinger wanted to have a wave, and in a classical picture, a wave is a wave. It has nothing to do with i. The i was just a mathematical trick. You calculate an AC circuit, and you use the i, and at the end you take the real part and that's the solution. So Schrödinger refused to have i. Do you know the story?

Huang: No, I don't.

Yang: The story is most interesting. I wrote a paper about it. What happened was, he formulates

$$H\psi = E\psi,$$

where

$$H = -\frac{\hbar^2}{2m}\nabla^2 + \cdots.$$

There's No i . He sent his manuscript to Lorentz, and Lorentz wrote back, saying that's very interesting, but I have a number of questions. And he listed — I forgot — fifteen questions. One of them says, you shouldn't have E in your formula. You don't have t, but you have E. You should have t without E, and E will come out of t, like in classical mechanics.

So Schrödinger thought about it, and of course eventually E becomes $i\hbar(\partial/\partial t)$. He didn't like that, because he didn't like the i. So what did he do? He fumbled over this for a few weeks, and one day he was extremely happy: He did it twice, and it worked! Starting with

$$i\hbar\frac{\partial}{\partial t}\psi = H\psi,$$

he took $i\hbar(\partial/\partial t)$ again, so

$$-\hbar^2 \frac{\partial^2}{\partial t^2}\psi = H^2\psi.$$

So, no more i, and he was extremely happy. (Laughter) How did we know he was happy? Because he wrote a letter to a friend — I forgot who — saying “A great load has been lifted off my shoulders.” (Laughter)

But then, a week after he wrote the letter, he found it didn’t work (Laughter), because if the Hamiltonian H contains time explicitly, there would be an additional term when you differentiate the second time. It was only then that he wrote down the i. It was not easy to accept the i (Laughter), and it happened only gradually.

Feynman made a great contribution, in my opinion. After another century, Feynman diagrams will probably be forgotten, but the path integral thing will last.

Huang: I believe experiments have a lot to do with it, because we have observed certain phases, beginning with flux quantization. And now with these electronic mesoscopic systems, we can manipulate the phase.

Yang: Sure. This is one of the fundamental things.

Huang: You devoted some effort to the quantum hard-sphere gas problem, in which I also participated. Can you comment on the significance of the solution for both Bose and Fermi statistics?

Yang: Mmm — Well, the most interesting case is Bose statistics. By the way, you know the first term for the energy, $4\pi a\rho$, is now claimed to have been rigorously proven by — (Huang prompted:) Elliot Lieb[25]. I took a look, but ... Did you understand his proof?

Huang: It’s not very transparent.

Yang: If correct, it’s a good contribution. I don’t know if it can be further pushed to the next term, I doubt it can be.

Huang: Well, Elliot told me he is working on the next term.

Yang: What we did in calculating the first two terms was in some sense —

You remember what happened. Luttinger and I were working on the problem, and we were not happy, because we were getting stuck, and getting nonsensical results. And you came to the Institute, and told me about this pseudopotential thing. I had vaguely read about it, but did not appreciate it. Coming from MIT and Weisskopf, you told me about it, and then we looked at it. The pseudopotential exactly cures the infinity we had encountered[26]. But I think the pseudopotential, though miraculous, is not likely to be the most important thing, because for the

[25] E. Lieb and J. Yngvason, *Phys. Rev. Lett.* **80**, 2504 (1998).
[26] K. Huang and C.N. Yang, *Phys. Rev.* **105**, 776 (1957).

lower-order calculation you do not need the pseudopotential. You only need the delta-function[27].

I am a little bit unhappy, and you must be too, that people have forgotten about our work. It is now called Pita... (Huang prompted:) Gross-Pitaevskii equation. But, anyway, let's break away from this discussion.

The Bose–Einstein condensation was a great discovery theoretically. The experimental discovery in 1995 was a miraculously amazing discovery. I would classify it as one of the subjects likely to become very important in the next five to ten years — especially the possibility of atomic lasers. That's another example how phase and coherence have dominated 20th-century physics, and will dominate 21st-century physics.

Huang: There are many calculation that have not been done, that probably could be done using the pseudopotential.

Yang: I strongly suspect that most could be done with the delta function. It is only when you want to go to a somewhat higher-order that the subtlety enters. That's why many people who enter the field recently don't know about the pseudopotential.

Huang: The method used in the hard-sphere problem was valid only near the ground state, and consequently it gives only low-temperature properties. Is there a way to treat the Bose gas near the transition point?

Yang: That's why Lee and I developed the binary-collision expansion method[28]. We were very enthusiastic about it. It's very elegant, but we were defeated by the subtlety of the transition point. I think in about 1958–59 I was hopeful we could disentangle the λ-transition, but we later gave up. It's a very subtle problem.

I suspect some new physical idea needs to be introduced before we can make headway. The binary-collision expansion method is formalistically very beautiful, but I was convinced, by the time I left it, that by itself it would not resolve the question of the nature of the λ-transition.

Huang: The binary-collision expansion method was mainly designed to treat singular potentials by summing diagrams. Is that right?

Yang: Yes

Huang: You mostly had the hard-sphere problem in mind?

Yang: Yes

Huang: It is still a perturbation expansion, with resummation of diagrams.

[27] The delta-function $g\delta(\mathbf{r})$ can be used to approximate real potentials in the low-energy region. The pserdopotential is the more accurate version $g\delta(\mathbf{r})(\partial/\partial r)r$, which avoids divergences in higher-order calculations.

[28] T.D. Lee and C.N. Yang, *Phys. Rev.* **113**, 1165 (1959).

Yang: Yes.

Huang: So maybe what you need, at the critical point, is another kind of resummation.

Yang: It could be. But I think this is simply related to the question you asked earlier. Such a transition is a collective phenomenon. Therefore, in whatever approach you make, that collective aspect, or enough of the collective aspect, has to be taken care of, before you can really master that transition. I don't know how that would come about. This is why after 1959–60 I completely moved away from the topic.

Then, of course, there was the excuse that nobody knew how to do the experiment. In fact, even as late as 1990, I saw there were people making noise that they were close to achieving Bose–Einstein condensation. I thought they were making propaganda. But then they did it in 1995. This shows how technology can really make great progress.

I don't know whether I told you the story. I first saw some of the equipment doing that at Stanford.

I said, "Where is your cryogenic equipment?"

I thought there would be a whole room of cryogenic equipment. (Laughter)

Huang: What about the variational principle on occupation numbers, which you also developed with T.D. Lee?

Yang: When we developed that, I though it was really elegant[29]. But, as I said, I think it's not enough. Theoretical physics develops sometimes because it is necessary to polish formulations so it becomes more and more elegant. But when it reaches a certain level, for deeper penetration you need additional things. I think that's not up to the task of treating the λ-transition.

Huang: You probably know that there is a way to derive the transition point of the two-dimensional Ising model without solving it exactly, by Kadanoff[30], and also Polyakov[31]. It only works near the transition point.

Yang: Did they get the 1/8? Did they get β =1/8?[32]

Huang: Yes.They also got the logarithmic specific heat. It is very simple, and does not involve any of this formal stuff.

Yang: I doubt they could get the 1/8. To get the logarithm is a bit simpler, because getting zero is easier then finding a number[33]. (Chuckle) I haven't looked into that.

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[29] T. D. Lee and C.N. Yang, *Phys. Rev.* **117**, 22 (1960).

[30] L. Kadanoff, *Phys. Rev. Lett.* **23**, 1430 (1969).

[31] A.M. Polyakov, *Gauge Fields and Strings* (Harwood Academic Publishers, Chur, Switzerland, 1987).

[32] Exact calaculations give the critical exponent $\beta = 1/8$ for the two-dimensional Ising model. In standard terminology, the order parameter (in this case, the magnetization,) vanishes like $|T - T_c|^\beta$, as the temperature T approaches the critical temperature T_c.

[33] In fact, Kandanoff did get $\beta = 1/8$.

Huang: It is in Polyakov's book on gauge field theory. The Kadanoff paper is very difficult to read. It's based on the question how many operators do you need to make a complete description. It predated the Wilson operator-product expansion. Anyway —

You did some work on a quantum hard-sphere system in the neighborhood of close packing. As far as I know, this was never published. Can you comment on this?

Yang: Yes. Let me see. I had thought that, starting from a completely stuck close-packing situation, you can loosen up a little bit. You can look at the $3n$-dimensional phase space. OK, let me recall.

When it is completely stuck, there is only one point in the phase space that is allowed. The question now is, if you loosen that up a little bit, what would happen. Then you realize that the point becomes a little volume. The little volume is bounded by quadratic surfaces, because spheres are quadratic surfaces. However, because they are so small, you can replace them by their tangent-planes. So the volume becomes a polyhedron, a $3n$-dimensional polyhedron. The reason I didn't publish anything was that I didn't know how to handle the polyhedron. I wanted to find the lowest mode.

The way to find the lowest mode in a polyhedron is by the method of mirror-imaging. For example, if I give you a triangle, and I want to find the lowest mode, one way is to reflect and invert, and then it becomes a hexagon. For example, a 45° right triangle becomes a square by reflection. If you know the solution of the square, you take the excited state of the square, and you get the solution for a triangle. So the standard method is by imaging. OK, but that polyhedron does not come back to itself when reflected. I tried many times, and after a while I got confused. So that is why it was never published.

I think it is an interesting mathematical problem.

Huang: It seems to be a well-defined mathematical problem. Maybe some mathematician can solve it.

Yang: You know it is not easy to find a mathematician who will sit down with you and listen to the end of what you are trying to say. (Laughter)

Yes, I did think about it for quite a while, but it was never published.

Huang: It would be extremely interesting if one can solve this problem, enough so we can meet the low-density end. Maybe it could be checked experimentally, with the new experiments on Bose–Einstein condensation.

Yang: I think the Bose–Einstein condensation is a marvelous discovery, which went through a series of four or five beautiful technological innovations. As I said, the field is likely to be most important in the next five to twenty years.

Huang: Do you believe it will lead to some important applications?

Yang: Yes, I'm convinced of that. Specifically, in time it will lead to atom lasers.

Huang: We already have atom lasers.

Yang: Rudiments of that.

Huang: Yes, rudiments of that.

Yang: Atom lasers are much more powerful then photon lasers, because it has internal degrees of freedom. Therefore you can manipulate it, and in that manipulation, you manipulate the phase in very complicated ways. When you can get a sizable atomic laser with intensity, it would be a new world for that type of experiments.

Huang: It's interesting that, when you talk to people about this, their reactions are different, depending upon what discipline they are in. Particle theorists mostly think this is not fundamental.

Yang: That this is not fundamental?

Huang: Yes, this is not fundamental. This is only a device.

Yang: Who is this?

Huang: Almost any particle theorist. You see, it's an understood phenomenon.

Yang: I'm an elementary particle theorist, and I am not of that opinion. I think there's jealousy in it. The truth is, elementary particle physics made great progress in the last century, or in the last 50 years, but its dominance of the publicity of physics is coming to an end.

I don't know whether you know this story — I think you were not there.

In 1980, I think, Marshak organized an international conference at VPI. I think you were not there. Marshall specifically organized it, partly because Zhou Guangzhao was visiting for a year, or a year and a half, and Marshak was a great admirer of Zhou Guangzhao. So he organized the conference, and many people were there. The last day, Saturday morning, was devoted to a panel discussion about the future of high-energy physics. Did you hear this story?

Huang: No.

Yang: Before that day, I had been asked to participate in the panel. I refused. I said I didn't think I had reasonable things to say. So I was sitting in the audience, and there was a panel on stage. Who was on the panel? Ten people: Marshak, T.D. Lee, Martin Perl, Gursey, Weinberg, maybe Glashow. Zhou Guangzhou? Oh yes, Nambu, and also some Europeans. There were two camps. One camp said W and Z would be discovered, and the other camp said W and Z would not be discovered, mostly in the tone that it's better for them not to be discovered, so you have some puzzle as to what's going on[34].

[34] The intermediate vector bosons $W^{\pm}$ and Z^0, which mediate the weak interactions, were experimentally discovered in 1983.

They talked for about an hour, and were near the end of the panel, when suddenly Gursey spotted me sitting in the front row.

He said, "Professor Yang is in the audience. We would like to hear his opinion."

I said, "No, no, I already declined to be on the panel."

But then everybody said they wanted me to say something. So, on the spur of the moment, I said to Marshak,

"Yes, I will say something, if you promise not to publish it".

He said OK, and he stuck to his word later.

So I said, "In the next ten years,"— I think the title of the panel was either the future or the next ten years of high-energy physics — I said, "In the next ten years, the most important discovery in high-energy physics is that 'the party's over'."

After I said that, there was general silence. Nobody said a word, and then Marshak declared the panel was finished. I remember immediately afterwards several young people surrounded me, in particular Henry Tye — Do you know Henry Tye?

Huang: Yes.

Yang: So Henry got into an argument with me, and I said, "I won't argue with you; but please remember, what I said to you is more important for your future then mine." (Laughter)

Huang: That's very true; but some people still believe it's not over.

Lets talk about one-dimensional systems. You used the Bethe hypothesis to solve the one-dimensional quantum hard-sphere gas[35]. Can this be duplicated in high dimensions?

Yang: I'm convinced it cannot be. What is the Bethe hypothesis? It really says that, for this many-body one-dimensional problem, there is no diffraction. There's only reflection. And because it's all reflection, you have a algebraic problem. If you have diffraction, it's an analytical problem. This allows you to solve the problem algebraically, and that is the basis for all these Bethe hypotheses.

Once you have more than one dimension, there has to be diffraction. So I don't believe the trick that works for one dimension would work in general.

By the way, you know that in Bethe's 1929 or 1930 paper[36], there was a footnote that said he was generalizing this to higher dimensions in a forthcoming publication?. The forthcoming paper never "forthcame," precisely because of what I said.

Huang: Does this mean that the property one finds cannot be generalized to higher dimensions?

Yang: Yes, that's right, because in higher dimensions you have an integral and not a sum, and at that point it becomes an analytical instead of algebraic problem. All the Yang–Baxter equations[37] capitalize on the algebraic property.

[35] C.N. Yang, *Phys. Rev. Lett.* **19**, 1312 (1967).

[36] H.A. Bethe, *Z. Physik* **71**, 205 (1931).

[37] The original Yang–Baxter equation was contained in C.N. Yang, *ibid.* It was obtained in a different context by D.J. Baxter, *Ann. Phys.* **70**, 193 (1972).

Huang: And yet the Yang–Baxter equation has far reaching application in other fields.

Yang: Yes, not only far reaching applications in other fields, but you can look at it his way. One of the great mathematical structures, starting with the end of the 19th century and the beginning of the 20th century, is the Bianchi identity — no, the Jacobi identity. The Jacobi identity has the structure

$$CC + CC + CC = 0$$

where the $C's$ are structure constants[38]. It is a three-term quadratic equation. Do you know this way of writing the Jacobi identity? The solution of this equation was found by Cartan, and that's how Cartan was led to the classification of all groups.

Huang: That is a trivial statement, if you start with commutators[39].

Yang: It is a trivial statement.

Huang: But it is not trivial, when you turn it around and ask (Yang chimed in, in unison:) given this, how many solutions there are.

Yang: It is not trivial (a) how to solve it, and (b) how that's related to the structure of Lie groups.

Huang: And how unique is the solution. Is that a relevant question?

Yang: Yes, it is a fundamental question. Now, I think the Yang–Baxter equation is so useful in so many different things, because it is a cubic equation. It captures, in some respect I think, the next generalization of greater complexity but in the same direction as the Jacobi identity. So you might say it is a generalized Jacobi identity. The ramifications of this are not yet completely understood.

Huang: You might say that, given the Jacobi identity, one way to represent it is through commutators.

Yang: Yes.

Huang: And that leads to Lie groups.

Yang: Yes.

Huang: What is the thing that Yang–Baxter leads to?

Yang: I don't know. It is certainly deeply related to Lie groups; but it is more complex, because it is a more complicated type of equation.

Huang: Well, do you know of any mathematical structure that is relevant?

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[38]The Jacobi identity is $C^n_{ab}C^d_{nc} + C^n_{bc}C^d_{na} + C^n_{ca}C^d_{nb} \equiv 0$, where the C's are structure constants in the Lie algebra $[L_a, L_b] = iC^c_{ab}L_c$, where L_a is a generator of the Lie group. Repeated indices are summed over their range.

[39]The commutator form of the Jacobi identity is $[[L_a, L_b], L_c] + [[L_b, L_c], L_a] + [[L_c, L_a], L_b] \equiv 0$.

Yang: One simple aspect of it is the — what is it called? — the knot theory. Knot theory is based on the equation

$$ABA = BAB$$

Now, the Yang–Baxter equation, in the simplest form, is this equation. This is a fundamental algebraic structure, a fundamental cubic structure. This simple equation was first found by Lifshitz, the great mathematician at Princeton. Do you know that it led to the Jones polynomial, and the great advancement of the theory of knots? — the classification of knots, for which Jones got the Field Medal? But $ABA = BAB$ is too simple. The Yang–Baxter equation is sort of the full-fledged equation[40].

Is there something like the Lie group, which was built on the Jacobi identity? You might say that the Lie group is a truly fundamental thing in 20th-century mathematics. Will something like it grow out of the Yang–Baxter equation? I don't know.

Huang: You can approach the Lie group from many different directions.

Yang: That's right.

Huang: Maybe there is already something, but we don't know the connection.

Yang: That's right.

Huang: It's remarkable that, by studying a one-dimensional fermion hard-sphere system, you can come up with something like this.

Yang: Well, I always say, if you study the simplest problems, you are likely to encounter important methods and important structures. What could be simpler then a one-dimensional many-body problem — especially a fermion one-dimensional problem?

How I got interested in the problem? Not because I was necessary interested in the statistical mechanics aspect. But the paper of Byers and myself, and then my paper on ODLRO, caused me in the early sixties to decide I needed a lattice model, a simple lattice model of fermions, which would show that you automatically have ODLRO. That led me to restudy the Bethe, Bloch-Bethe papers on the Heisenberg model. Once I got into that, I got deeply involved in the Bethe method.

Do I have regrets for getting involved in that, of course not.

What I regret is the following. In about 1970— I think my paper on Yang–Baxter was published in 1967 — there were many postdocs at Stonybrook. Somehow around that time I abandoned that subject, partly because I got interested in gauge fields again.

I have never been a person who discusses with many people — you know my style. I would find something of interest to discuss with one colleague, and then

[40] The Yang–Baxter equation is the matrix equation $A(u)B(u+v)A(v) = B(v)A(u+v)B(u)$, where $A(u)$ and $B(u)$ are matirix functions.

we would closely collaborate, and forget about everybody else. That was also true with my graduate students, but of course that was wrong.

In 1967, Stonybrook was one of the hottest centers for this type of thing. I should have asked many people, and say you do this, you do this, and then we would have built a school. The only interested person I induced into that field was Hans Thacker. I don't know if you know him. He is now at Virginia. He did very well in the field, but he was no match for this army of Russians (Chuckle), who in the seventies greatly developed the field[41]. They made all kinds of new models, and each one was solved by some version of the Yang–Baxter equation. So by 1980 I realized that I didn't do it correctly. I should have gotten many post docs, and many graduate students. In the seventies, the Russians took over the whole field.

But I guess everybody has his own style, and it is not something you can change easily, and also I didn't realize that the area would develop so dramatically. (Chuckle)

Huang: Do you believe there really are any physical one-dimensional systems — a system that can be realized in the laboratory, and that you would say, yes this is a one-dimensional system?

Yang: The one great topic the material scientist and the surface scientist are most interested in, of course, is two-dimensional systems. But they are also interested in one-dimensional systems. So the answer to your question is, "certainly."

I think even not from the theoretical viewpoint, but just the experimental viewpoint, low-dimensional systems, two and one-dimensional systems, are becoming more and more important. You know of course Bill Little had already conjectured in the sixties, that some one-dimensional systems would become high-temperature superconductors. That has not completely materialized, but it is still a very interesting suggestion.

[41] A.H. Belavin, A.A. Polyakov, and A.B. Zamoldodchikov, *Nucl. Phys.* B **241**, 333 (1984) developed what is known as "conformal field theory".

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